The Great Rewriting

The Classroom as a Battleground

Contents

Part I — The Rebranding
Chapter 1. The 2024 NCERT Curricular Shift
Chapter 2. The Machinery of Curriculum

Part II — Chronology
Chapter 3. The Problem of the Ratha
Chapter 4. The Missing Horse
Chapter 5. The Ayas Distinction

Part III — The Sarasvatī Question
Chapter 6. The Sarasvatī Paradox
Chapter 7. The Helmand-Harakhvaitī Hypothesis
Chapter 8. The Anti-Clockwise Mapping

Part IV — Language as Evidence
Chapter 9. The Substrate Argument
Chapter 10. Temperate Words in a Tropical Land
Chapter 11. Beaver to Mongoose

Part V — Method and Motive
Chapter 12. The Bardic Tradition
Chapter 13. The Status Kit Model
Chapter 14. Scientific Cultism

Part VI — Other Erasures
Chapter 15. The Baudhayana-Pythagoras Question

Part VII — Stakes
Chapter 16. Summary of Contradictions
Chapter 17. Why the Classroom Is the Battleground

Further Reading

About This Book

In 2024, India's National Council of Educational Research and Training rewrote a name. The Indus Valley Civilisation, taught for a century under that label, became the "Sindhu-Sarasvatī" or "Indus-Sarasvatī" Civilisation in revised NCERT textbooks. A name change sounds like a small thing. It is not. Embedded inside the new label is a historical claim: that the people who built the cities of Mohenjo-daro and Harappa were the same people, or the direct cultural continuers of the same people, who composed the Rigveda beside a river called the Sarasvatī. That claim, if true, would collapse a distinction that a century of archaeology, philology, and comparative linguistics has treated as basic: that the urban Harappan world and the pastoral, horse-centered world of the Rigveda are separated by geography, technology, and several centuries of time.

This book began as a single investigative report examining that rebranding against the chronological, hydrological, linguistic, and historiographic record. What follows is that report expanded — every claim it made pulled out into its own chapter, every thread followed further than a report's length allows, and the institutional question of how and why a government curriculum body arrives at such a rewrite given its own chapter. A later addition — Part VI — steps outside the Sindhu-Sarasvatī case specifically to ask whether the same narrative habit shows up elsewhere in how Indian curricula and popular science writing tell the story of ancient Indian mathematics. Seventeen chapters, organized into seven parts, each built around a single theme: a piece of technology, a river, a word, a modeling assumption, a satellite photograph, an institution, a theorem.

Two things are worth saying plainly before the first chapter begins.

First, this is not a manufactured controversy. The question of who composed the Vedic hymns, when, and in what relationship to the Indus cities is one of the oldest live debates in South Asian studies, running from colonial-era philology through today's ancient-DNA laboratories. Serious, credentialed scholars have staked out positions on both sides of it — the migrationist mainstream, sometimes called the Aryan Migration Theory (AMT), and a minority indigenist position often called the Out of India Theory (OIT), which holds that Sanskrit and the Vedic culture are native to the subcontinent and that Indo-European languages radiated outward from India rather than into it. This book argues the AMT case, and argues it with intent to persuade — it does not pretend to be a disinterested survey. But because the debate is real, four of the seventeen chapters — the ones concerning the Sarasvatī's hydrology and the linguistic substrate evidence, where the indigenist case is strongest — pause to lay out the best version of the opposing argument before answering it. A reader who wants only one side's version of those four chapters is free to skip the boxed sections marked "The Other Case." A reader who wants to know whether this book's confidence is earned should read them.

Second, a note on what "government" and "education mistruths" mean in this book. No living official is named or accused of personal wrongdoing here. The critique is institutional: how a textbook-writing process can promote a minority scholarly position to settled fact in a document that eleven-year-olds will memorize and rarely revisit. That

critique would apply, in principle, to a curricular body anywhere that did the same thing in the other direction — and the book says so, in Chapter 2, before it says anything else.

With that framing, the investigation begins where the report began: with the year 2024, and a name.

Part I — The Rebranding

Chapter 1: The 2024 NCERT Curricular Shift

Names are never neutral in historiography. When a body of water, a people, or a civilisation gets a new label in a textbook, the label is doing argumentative work whether or not the students copying it into their notebooks realize it. "Indus Valley Civilisation" is itself a name with a history — it emerged in the 1920s, after the excavations at Harappa and Mohenjo-daro, and it described the civilisation by the river system along which its great cities were found. It made no claim about who those city-builders spoke to, worshipped, or were ethnically descended from. It was, deliberately, a geographic label for an archaeological culture whose language remains undeciphered to this day, because the Indus script has never been convincingly read.

"Sindhu-Sarasvatī" does something the older name never did: it makes a linguistic and religious claim under the guise of a geographic one. "Sindhu" is simply the Sanskrit-derived name for the Indus, so the first half changes little. The addition of "Sarasvatī" is the substantive move. The Sarasvatī is not a name archaeologists gave the Indus civilisation from external evidence; it is the name of a river celebrated in the Rigveda, the oldest layer of Vedic literature, as sacred, mighty, and central to the world of the RV's composers. By fusing "Sindhu" and "Sarasvatī" into a single civilisational label, the new NCERT nomenclature asserts — before a single piece of evidence has been presented to the student — that the Harappan cities and the Rigveda's world are one continuous culture, viewed from two angles. It resolves, by naming convention, a question that professional archaeology and historical linguistics have not resolved by evidence.

This is what "rebranding" means in this book's title, and it is worth being precise about why it matters more than an ordinary academic disagreement would. Most scholarly disputes stay inside journals and conferences; specialists argue, evidence accumulates or doesn't, positions shift over a generation. Curriculum works differently. A textbook claim is not presented to its reader as one hypothesis among several — it is presented as the settled starting point from which further learning proceeds. A fourteen-year-old who is taught that the Sindhu-Sarasvatī Civilisation flourished along a sea-going Sarasvatī river inhabited by proto-Vedic Aryans will, in the overwhelming majority of cases, never revisit that claim as an adult. It becomes background knowledge — the kind of fact one doesn't remember learning, because one never consciously learned it; one simply always seems to have known it. That is what makes curricular nomenclature disputes higher-stakes than academic ones: they aim not at winning an argument among peers who can weigh counter-evidence, but at installing a conclusion in minds that have no counter-evidence to weigh it against.

The chapters that follow examine, piece by piece, whether the conclusion embedded in "Sindhu-Sarasvatī" survives contact with the archaeological, hydrological, and linguistic record. The short version, which the rest of this book will justify at length, is that it does not survive contact easily. The Indus cities were an urban Bronze Age culture that had already gone into terminal decline by roughly 1900 BCE. The horse-and-chariot culture reflected throughout the Rigveda does not appear in the South Asian archaeological record until thereabouts three to five centuries later. The river the Rigveda calls mighty and

sea-reaching corresponds, when checked against the region's actual hydrology and geomorphology, to a system that was already desiccating or had already lost its glacial and monsoon-fed tributaries well before the most probable dates for the hymns' composition. And roughly 380 words embedded in the Rigveda itself — including basic vocabulary for the plough, for regional names, for at least one tribal leader's name — do not fit the sound-system of Indo-European languages at all, suggesting the Vedic-speaking population arrived to find people already living there, speaking something else, and borrowed words from them as newcomers do.

None of this makes for a tidy origin story, which may be part of why a tidier one is politically appealing. A civilisation that is simply, continuously, and uncomplicatedly "us" — no migration, no linguistic layering, no substrate populations whose languages left fingerprints in the sacred text — offers a cleaner claim to unbroken indigenous continuity than the messier, migration-and-mixture picture that the evidence actually supports. But a textbook's job is to teach the messier picture when the messier picture is what the evidence shows, and to teach it as contested where it is genuinely contested, rather than resolve the contest by fiat inside a compound noun.

The remainder of Part I turns from the claim itself to the process that produced it: what NCERT is, how a name like this makes it into a printed textbook, and what "getting it wrong" costs when the getting-wrong happens at the level of a national curriculum rather than a single scholar's paper.

Chapter 2: The Machinery of Curriculum

It is tempting, and too easy, to treat a rebranding like this as the isolated decision of a single official signing off on a single document. In practice, national curricula are produced by committees, review cycles, subject-expert panels, and a chain of institutional approvals — and understanding that machinery matters, because it explains both how a minority scholarly position can end up presented as consensus, and why the resulting document reads as authoritative even when the underlying claim is contested.

NCERT — the National Council of Educational Research and Training — is the central body responsible for developing the model curriculum framework and textbooks used across a large share of India's school system. Its textbooks are drafted by subject-expert committees, reviewed internally, and revised periodically to reflect updated National Curriculum Frameworks. This is, in principle, a reasonable structure: subject experts, not politicians, are supposed to write the content, and revision cycles exist precisely so that outdated or superseded material can be replaced with better-supported material as scholarship advances.

The trouble is that a committee structure filters expertise through selection. Who gets appointed to a subject-expert panel, which scholars' work that panel treats as authoritative, and which framework document sets the panel's brief all shape the output well before any individual sentence is drafted. A panel weighted toward scholars sympathetic to indigenist historiography will treat the "Sindhu-Sarasvatī" continuity thesis as the more natural starting point; a panel weighted toward the migrationist mainstream would have produced a different textbook using the same archaeological finds. Committee composition is itself a policy lever, and it is one that operates upstream of any individual claim a textbook makes — which is why auditing the claims a textbook makes, chapter by chapter as this book does, is a necessary check regardless of which committee produced them.

It is worth being fair about the historical symmetry here, because this book's critique loses force if it pretends the problem is unique to one ideological direction. Indian textbooks have carried contested framings before the 2024 revision, and the sources of contestation have not run in only one political direction. Marxist historiographical assumptions shaped earlier generations of NCERT history textbooks in ways that some scholars — across the ideological spectrum — have also flagged as presenting one interpretive framework as settled fact, particularly around questions of caste, feudalism, and the economic character of pre-colonial India. The mechanism this book is describing — a committee's interpretive lean becoming a textbook's declarative sentence — is a structural feature of how curriculum bodies work everywhere, not a defect invented in 2024. What makes the present case worth an entire book is not that institutional bias exists (it is close to unavoidable in any collective drafting process) but that this particular rebranding happens to collide with an unusually rich, unusually well-dated, and unusually cross-checkable body of archaeological and linguistic evidence — which makes it possible to show, chapter by chapter, exactly where the textbook's claim and the evidentiary record diverge.

That is the sense in which this book uses the phrase "education mistruths." It does not mean deliberate lying by any named individual. It means a structural failure mode: a

curriculum document stating, in declarative sentences with no attributed uncertainty, a claim that specialist literature treats as one contested position among several. A textbook that wrote "some scholars argue the Sarasvatī river of the Rigveda corresponds to the Ghaggar-Hakra system, though its dating relative to the hymns remains disputed" would be teaching contested history honestly. A textbook that folds the same claim into a civilisation's proper name teaches it as though the dispute were already resolved. The difference between those two sentences is the entire subject of this book.

Why does this matter beyond the classroom? Because curricular claims, unlike journal-article claims, propagate without a citation trail. A journal article that overstates its evidence gets challenged in peer review, in follow-up papers, in conference rebuttals — the scholarly correction mechanism, imperfect as it is, exists and functions over years. A textbook claim, once printed and distributed to tens of millions of students, has no equivalent correction mechanism operating on the same timescale. It gets revised, if at all, on the multi-year cycle of the next curriculum framework, by which time an entire cohort has already absorbed the earlier version as background knowledge. This is the asymmetry that makes curricular nomenclature disputes worth a sustained investigation rather than a one-paragraph objection: the claim is cheap to insert and expensive to correct.

The chapters that follow are organized to make correction cheap, at least for the individual reader. Each one isolates a single strand of evidence — a technology, a faunal record, a metallurgical term, a river's geomorphology, a hymn's geography, a word's phonology, a semantic drift, a modeling framework, a satellite photograph — and asks whether that strand supports the continuity claim embedded in "Sindhu-Sarasvatī" or contradicts it. Part II begins with chronology, because chronology is where the rebranding's central claim is most directly testable: does the archaeological date range of the Indus cities overlap with the archaeological date range of the horse-and-chariot culture the Rigveda describes throughout its hymns?

Part II — Chronology

Chapter 3: The Problem of the Ratha

Open the Rigveda anywhere in its earliest layers and the chariot is close by. The word is *ratha* — a light, horse-drawn, spoke-wheeled vehicle — and it appears not as an occasional curiosity but as the central image of Rigvedic poetry: gods ride chariots across the sky, warriors ride them into battle, hymns are structured as races between them, and the vocabulary of the text is saturated with chariot-part terminology precise enough that philologists have reconstructed the vehicle's engineering from the poetry alone. This is not a culture that mentions the chariot. It is a culture organized, imaginatively and materially, around the chariot.

That matters because the spoke-wheeled chariot has an archaeological birthdate, and it is a comparatively well-established one. The technology — a light wheel built from a hub, spokes, and a rim, dramatically lighter and faster than the solid-wheeled carts that preceded it — first appears in the archaeological record on the Eurasian steppe, in sites associated with the Sintashta culture of the southern Urals, dated to roughly 2100–1800 BCE. From there, the technology diffuses outward across Eurasia over the following centuries, reaching different regions at different times as it travels with the populations and trade networks that carried it.

In South Asia specifically, the earliest archaeological evidence for horse-drawn, spoke-wheeled chariots — and for horses used culturally in this way at all — appears at sites in the Swat Valley and at Pirak, in the northwestern reaches of the subcontinent, dated to a window of roughly 1700–1400 BCE. This is not a marginal or disputed dating; it reflects a broad convergence across excavation reports from the region.

Now set that date range beside the chronology of the Indus Valley Civilisation's urban phase. The Mature Harappan period — the era of planned cities, standardized brick sizes, granaries, drainage systems, and the still-undeciphered Indus script — is conventionally dated to roughly 2600–1900 BCE. By the time the archaeological signature of horse-drawn chariots begins appearing in the northwestern subcontinent, the great urban centers of the Indus system had already been in decline, or had already been abandoned, for a minimum of two centuries — and by many chronologies, for closer to four or five.

This is the chronological seam that the "Sindhu-Sarasvatī" rebranding has to paper over, and it cannot be papered over by pointing to the eventual overlap between the two cultures' broader geographic footprint, because geographic overlap and temporal overlap are different claims. The Rigveda's world and the Harappan world may occupy some of the same river valleys. They do not, on the current archaeological dating, occupy the same centuries. A culture whose poetry is structurally inseparable from the chariot cannot have been the same culture, at the same time, as an urban civilisation that shows no chariot technology in its archaeological strata and had already entered terminal decline before that technology's earliest confirmed regional appearance.

There is a way to try to save the continuity thesis here, and it deserves to be stated rather than ignored: one could argue that the *ratha* culture of the Rigveda represents a later phase of a continuous indigenous population, so that "Sindhu-Sarasvatī" describes an evolving civilisation across a very long span — urban Harappan first, chariot-riding Vedic later, same underlying people throughout. This is a coherent position in the abstract. But it requires the chariot-and-horse complex to be an indigenous South Asian development rather than an import from the steppe corridor — and the archaeological and faunal record, taken up in the next chapter, does not support an indigenous origin for either the horse or the spoked chariot wheel in this region. The technology arrives from outside, on a timeline that trails the Harappan urban collapse rather than coinciding with its peak, which is a pattern more consistent with a later arriving population bringing a new technology than with a single continuous culture inventing it locally partway through its own history.

The chariot, in other words, is not a decorative detail this investigation happens to notice. It is a load-bearing piece of the Rigveda's entire self-presentation, and its archaeological birthdate falls squarely after the Indus cities had already fallen quiet.

Chapter 4: The Missing Horse

If the chariot is the Rigveda's central image, the horse is its central animal — and unlike some archaeological questions, this one can be checked directly against the bones. Zooarchaeology, the study of animal remains recovered from excavation, can in principle answer a simple factual question: were horses present, in numbers and contexts suggesting cultural centrality, at Harappan sites during the civilisation's urban peak?

The answer, across the great majority of excavated Mature Harappan sites, is no. The faunal assemblages recovered from cities like Mohenjo-daro, Harappa, and Dholavira are dominated by cattle — humped zebu cattle above all, alongside water buffalo, sheep, and goat — animals consistent with an agrarian, cart-and-plough economy rather than a horse-centered pastoral or martial one. The bull appears repeatedly in Harappan seal iconography, elevated to what looks like ritual or symbolic importance. The horse does not appear in the seal corpus in any unambiguous, uncontested depiction, and confirmed horse skeletal remains from securely dated Mature Harappan contexts are, at best, exceptionally rare.

This is worth stating carefully, because the horse-bone question has not been entirely free of controversy on its own terms. A small number of sites — Surkotada being the most frequently cited — have produced faunal material that excavators at various points identified as possible horse or horse-like equid remains from Harappan-period layers. Those identifications have been revisited by subsequent zooarchaeological review, and the consensus that has emerged treats most of the disputed material as either wild ass (onager), a similar equid species native to the region, or as too fragmentary and ambiguous to sustain a confident species identification — not as settled evidence of a horse-centered Harappan culture. Genuine, well-attested, unambiguous domesticated horse remains in South Asia begin appearing reliably only from around 1700 BCE onward, clustering in the same northwestern sites — Swat Valley, Pirak — that produce the earliest chariot evidence

discussed in the previous chapter. Horse and chariot arrive together, as a package, and they arrive after the Harappan cities were already in decline.

Why does this matter so much for a civilisation the Rigveda constantly celebrates? Because the horse is not incidental to Vedic religion — it is central to it. The Rigveda contains extended hymns to horses; the *Ashvamedha*, the horse sacrifice, is one of the most elaborate ritual complexes described in the wider Vedic corpus and remained a marker of royal legitimacy for centuries afterward. A culture that ritually venerates the horse at this level of intensity, and structures its central sacrificial rite around the animal, is a culture for which the horse was not a peripheral resource but an organizing symbol of status and divinity. That is precisely the kind of cultural centrality zooarchaeologists describe as part of the Indo-European "prestige status kit" — horse, chariot, and the ritual complex built around both — a package with a traceable steppe origin and a traceable, dated diffusion path into South Asia.

The absence of that package from the Harappan archaeological record, across a civilisation excavated at dozens of major sites over a full century of fieldwork, is not an argument from silence in the weak sense. It is an argument from a very large, very intensively studied body of positive evidence — cattle bones, cattle seals, cart imagery, granary architecture — that consistently points toward a different economic and symbolic center of gravity than the one the Rigveda describes. If the Harappans and the Rigveda's composers were the same culture at the same time, the horse's near-total absence from one and near-total centrality to the other would be an extraordinary coincidence requiring its own explanation. The far simpler explanation, consistent with the chariot chronology in the previous chapter, is that they were not the same culture at the same time: the horse-and-chariot people arrived later, into a landscape whose great cities had already gone quiet.

Chapter 5: The Ayas Distinction

The Rigveda dates itself, in a sense — not with a calendar, but with metal. The word *ayas* appears throughout the earliest hymn layers, and philological analysis of its usage shows it referring exclusively to copper or bronze: a Chalcolithic or Bronze Age vocabulary, consistent with a culture that has not yet encountered iron as a working metal. Later in the broader Vedic textual tradition, a new compound appears — *śyāma ayas*, literally "dark metal" or "black metal" — used to denote iron specifically, distinguishing it from the older, undifferentiated *ayas*. This distinction did not need to be invented; it needed to be observed, because the compound only becomes necessary once a second metal exists that has to be told apart from the first.

Śyāma ayas emerges in textual layers that comparative Vedic philology dates to roughly 1200–1000 BCE — a window that lines up closely with the archaeological horizon for iron's introduction and spread across northern India, associated with the Painted Grey Ware culture of the Ganga-Yamuna doab region. This is a genuine convergence between two independent lines of evidence, textual and archaeological, arrived at by entirely different methods: philologists tracking vocabulary layers inside the Vedic corpus, and archaeologists tracking metallurgical residue and artifact typology in excavated strata. They

agree, within a reasonable margin, on when iron entered the northern Indian cultural world.

What this metallurgical layering does to the "Sindhu-Sarasvatī" rebranding is subtler than the chariot-and-horse argument, but arguably more damaging to the specific claim the name makes. The rebranding treats "the Vedic period" as though it were a single moment — a snapshot in time that can be fused with the Bronze Age Harappan world into one compound civilisational label. But the metallurgical vocabulary shows that the Rigveda itself is not a snapshot. It is a composite text, composed and orally transmitted across a span of centuries, whose earliest layers speak the language of the Chalcolithic and whose later layers — still inside the broader corpus this civilisational rebranding is drawing on — speak the language of the early Iron Age. A civilisational label that treats "Vedic" as a fixed point contemporaneous with Mature Harappan urbanism (2600–1900 BCE) has to somehow account for a text whose own internal vocabulary keeps evolving for a further six to nine centuries beyond that urban civilisation's collapse.

Put the three chronological chapters together and the shape of the problem becomes clear. The chariot technology central to the Rigveda's imagery appears in the South Asian archaeological record centuries after Harappan urbanism had already ended. The horse, equally central to Rigvedic religion, is essentially absent from Harappan faunal assemblages and only becomes archaeologically visible on the same later timeline as the chariot. And the metallurgical vocabulary embedded in the Vedic corpus itself shows internal development running from a Bronze Age idiom through to an Iron Age one — a span of composition that, at its latest end, sits closer to 1000 BCE than to the Harappan urban peak nearly two thousand years earlier. None of these three independent lines of evidence — technological, faunal, or philological — locate the Rigveda's world inside the same centuries as the Indus cities' urban florescence. All three place it afterward, by margins measured in centuries rather than decades.

This is the chronological foundation the rest of the book builds on. Part III turns from *when* to *where* — from the dating problem to the river the rebranding names itself after, and to the geological and hydrological record of whether that river was ever the sea-going, civilisation-defining waterway the new curriculum implies it was.

Part III — The Sarasvatī Question

Chapter 6: The Sarasvatī Paradox

The Rigveda does not mention the Sarasvatī in passing. It is addressed directly, repeatedly, and with a specific vocabulary of grandeur: the mightiest of rivers, the best of mothers, the best of goddesses, a river whose praise-hymns place it at the geographic and spiritual center of the world the poets describe. Some later interpretive traditions, and some readings of individual hymn passages, treat the Sarasvatī as reaching or bordering the sea. It is this image — a mighty, central, sea-reaching river, the defining geographic feature of the civilisation that produced the Rigveda — that supplies the second half of the "Sindhu-Sarasvatī" name and does much of the rebranding's persuasive work. If the Sarasvatī really was a great, perennial, sea-going river running alongside the Indus system during the Harappan urban period, then a civilisation named for both rivers together starts to sound plausible on its own terms.

The geological and hydrological record does not cooperate with that image as cleanly as the rebranding needs it to. The river system most commonly identified with the Rigvedic Sarasvatī is the Ghaggar-Hakra — today a seasonal, largely dry channel running through parts of Haryana, Punjab, and into the Thar Desert region of Rajasthan and Pakistan's Cholistan, with a well-documented paleochannel visible in satellite imagery and confirmed by ground survey. That the Ghaggar-Hakra was once a larger, more consistently flowing system than it is today is not seriously disputed; the paleochannel evidence for that is solid. The dispute — the genuinely contested part — is about timing. When did this system stop being a large, glacier- and monsoon-fed perennial river and become the seasonal, seepage-fed channel it largely is now? And does that timing overlap with the most probable window for the Rigveda's composition?

The weight of the geomorphological evidence points toward an earlier desiccation than the rebranding's narrative requires. A combination of tectonic shifts, river-capture events, and long-term climatic drying appears to have progressively cut the Ghaggar-Hakra off from its major glacial feeder — most likely an ancestral course of the Sutlej — pushing the system toward its present seasonal state well before the most probable dates now assigned to Rigvedic composition, which cluster in the mid-second millennium BCE, roughly 1500–1200 BCE, based on independent linguistic and comparative-philological dating discussed further in Part IV. If the major desiccation substantially predates the hymns, then the river the Rigveda praises as mighty and sea-reaching cannot straightforwardly be the same physical river system in the same physical state the poets are describing — which is, in outline, the paradox this chapter is named for: a text describing a river in terms that do not match the documented late-stage hydrology of the channel most often identified with it.

> **The Other Case.** Proponents of an earlier, indigenist chronology do not simply wave the hydrological evidence away, and it would be unfair to present their position as though they did. Several geomorphological and OSL-dating (optically stimulated luminescence) studies of the Ghaggar-Hakra basin have argued that portions of the system, or specific channel segments, retained substantial glacier-fed flow considerably later than the more conservative desiccation

timeline assumes — into the early second millennium BCE, and by some readings closer to 2000 BCE than 2500 BCE. Combined with an argument, made independently by a minority of linguists, that Rigvedic composition could plausibly be pushed earlier than the 1500 BCE mainstream estimate — closer to the third millennium BCE in the most maximalist versions — this creates a possible window in which a still-substantial Sarasvatī and an actively composing Rigvedic tradition could, in principle, overlap. This is the strongest form of the continuity argument, and it deserves to be engaged rather than dismissed by assertion.

It runs into two difficulties that have kept it a minority position rather than a new consensus. First, the OSL and geomorphological studies supporting a later desiccation date are contested within the geological literature itself, and the more widely replicated dating places the loss of major glacial input earlier rather than later — meaning the "later desiccation" reading is itself the less conservative of two live geological positions, not an agreed baseline being denied. Second, and more decisively, the early-composition dating for the Rigveda runs against comparative Indo-European linguistics independent of the river question entirely — the internal relationship between Vedic Sanskrit, Avestan, and the wider Indo-Iranian and Indo-European family tree constrains how early Vedic Sanskrit could plausibly have split off and still show the specific shared innovations and divergences it shows relative to its sister branches. Pushing Rigvedic composition back by half a millennium or more to rescue the river's timing creates new problems in the linguistic family tree that the river hypothesis alone cannot solve. The overlap window survives only if two independently contested claims — a later desiccation date and a much earlier composition date — are both true simultaneously, which is possible but is asking the evidence to cooperate twice rather than once.

The Sarasvatī paradox, then, is not that the river never existed or was never significant — the paleochannel is real, and the river's memory clearly mattered enormously to the culture that praised it. The paradox is that the specific claim embedded in "Sindhu-Sarasvatī" — a sea-going, civilisation-defining river contemporaneous with Harappan urbanism — requires a specific alignment of two separately uncertain timelines, geological and linguistic, that the balance of current evidence does not comfortably support. The next chapter asks a different but related question: even setting the river's physical state aside, does its name tell us where the culture that named it came from?

Chapter 7: The Helmand-Harakhvaitī Hypothesis

River names travel. This is one of the best-attested patterns in the study of migration and colonization worldwide: people moving into new territory frequently name unfamiliar landscape features after remembered ones from the land they left. New England is dense with towns named for English counties and cities their settlers had left behind; Cambridge, Massachusetts sits nowhere near the Cambridge its founders knew, and no one seriously argues its existence proves the Charles River was always called the Cam. The pattern is called hydronymic transference, and it shows up again and again wherever population

movement can be independently confirmed by other evidence — which makes it a useful diagnostic tool precisely because a name's presence in a new place is evidence about the namers' origin, not evidence about the landscape's own prior history.

The name Sarasvatī has a very close cognate outside India, and it sits exactly where the Indo-Iranian linguistic family predicts an ancestral homeland connection would place it. In Avestan — the language of the Zoroastrian scriptures, and Sanskrit's closest surviving linguistic relative — the corresponding form is *Haraxvaitī*, the name of a river and region in what is now southern Afghanistan, in the historical territory the Greeks called Arachosia, centered on the Helmand river system. Both names derive from the same Proto-Indo-Iranian root, roughly meaning "possessing waters" or "rich in ponds/lakes" — a plausible descriptive name for more than one river, but a suspiciously exact linguistic match to have arisen twice, independently, by coincidence. A second, parallel case reinforces the pattern: the Rigvedic *Sarayu* corresponds closely to the Avestan *Harōiiu*, the river name underlying the region and city of Herat, in western Afghanistan.

What makes this pairing more than a curiosity is where both Iranian-side rivers actually terminate. The Helmand system, like the Ghaggar-Hakra on the Indian side, does not reach an open ocean — it drains into the Hamun wetlands, an inland desert delta on the Afghan-Iranian border, a terminal basin rather than a sea-going outlet. That is a strikingly close geomorphological echo of the Ghaggar-Hakra's own terminal delta near Fort Derawar in the Cholistan desert, where the Indian-side system likewise dissipates into desert sands rather than reaching the sea. Two rivers bearing linguistically cognate names, on either side of the Indo-Iranian family's geographic range, both terminate in inland desert deltas rather than oceans. That symmetry is difficult to explain as coincidence and easy to explain as the same naming tradition, carried by a moving population, applied twice to two different rivers that happened to share the same terminal-delta hydrology the name's root describes.

> **The Other Case.** A fair objection here is that shared vocabulary does not by itself prove which direction a name traveled — or that it traveled at all. Two branches of a language family that inherited a common word for "a river rich in still water" could, in principle, apply that inherited descriptive term independently to two different rivers that happen to share that hydrological feature, without either naming event being a memory of the other. Root cognation proves common inheritance; it does not, on its own, prove that the Indian instance is the copy and the Iranian instance is the original rather than the reverse, or that neither is a "copy" of the other in any meaningful sense.
>
> This is a genuinely valid caution, and it is why this book does not rest the migration argument on the river names alone. The direction-of-transfer conclusion — that the naming tradition moved from the Iranian plateau into India, rather than the other way around, or arose independently in both places — depends on triangulating the river-name evidence against the other chapters in this part and the next: the archaeological corridor described by the Nadi-stuti's geography in the following chapter, and the substrate-vocabulary evidence in Part IV showing the incoming language absorbing local non-Indo-European words as it moved into a populated landscape, which is a pattern consistent with arrival

rather than indigenous origin. No single hydronym settles the direction of travel by itself. The convergence of the hydronym, the geography, and the substrate vocabulary together is what does the work — which is precisely the kind of cumulative case this book is built to present chapter by chapter rather than claim in a single line.

Chapter 8: The Anti-Clockwise Mapping

If a poetic tradition is composed by a people already settled around a single, central river — the Sarasvatī-centric picture the rebranding implies — its geography should read like the view from that center outward: the home river described first and most intimately, more distant rivers appearing as periphery. If instead the tradition is composed by a people moving into a new region from a specific direction, its geography should read differently: like a route, with the earliest-known landmarks appearing first and the poets' geographic horizon expanding as their world does.

The *Nadi-stuti*, the Rigveda's "river hymn," is the single passage most useful for testing which of these two patterns actually appears in the text, because it does what its name says — it praises rivers, in a sequence. That sequence runs, broadly, from the far northwest toward the southeast: rivers corresponding to the Kabul (Kubhā), the Kurram (Krumu), and the Gomal (Gomatī) — all tributaries associated with the Afghan highlands and the northwestern frontier — appear ahead of, or alongside, rivers further into the Punjab and toward the Sindhu (Indus) itself. This is not the geographic signature of a settled population describing its home territory outward from a center. It is closer to the signature of a population whose geographic horizon originates in the northwest and extends progressively southeastward — consistent with, though not conclusive proof of, a migration route running from the Afghan highlands down into the Punjab plains, the same general corridor the archaeological chariot-and-horse evidence in Chapter 3 traces on independent grounds.

A purely Sarasvatī-centric point of origin struggles to explain this directional bias on its own terms. If the poets' ancestral, generations-deep homeland were the Sarasvatī basin itself, the expectation is a geography radiating outward from that river as the emotional and descriptive center of gravity, with the northwestern tributaries appearing as comparatively distant, peripheral features. What the hymn actually shows is closer to the reverse: a northwestern point of reference that reads as earlier and more intimately known, with the world extending southeastward from it — the "anti-clockwise" or, more precisely, northwest-to-southeast trajectory this chapter is named for.

The Other Case. The most serious objection to reading the Nadi-stuti as a migration record is methodological, and worth taking seriously on its own terms: hymns are ritual poetry, not travel diaries or administrative surveys, and their internal ordering may reflect ritual sequence, poetic meter, trade-route familiarity, or purely compositional convention rather than literal lived memory of a journey. A river hymn's order proves, at most, that the poets found it natural to praise rivers in that sequence — not that the sequence encodes an ancestral migration

route, and reading migration into poetic ordering risks over-interpreting a text for a purpose it was never trying to serve.

That caution is fair, and this book does not treat the Nadi-stuti's ordering as proof by itself — no single hymn is asked to carry that weight. What makes the directional pattern worth including as one chapter among sixteen, rather than discarding it as too speculative to use, is that it does not stand alone. It converges with an archaeologically independent migration corridor (the chariot-and-horse technology entering the subcontinent from the northwest, described in Chapter 3), a linguistically independent hydronym pair pointing toward an Iranian-plateau origin (Chapter 7), and a substrate-vocabulary pattern consistent with a language arriving into an already-populated landscape (Part IV). A single poetic hymn's word order, read alone, would indeed be thin evidence. The same word order, read as one data point among four independently derived lines pointing the same direction, is a different kind of evidence — and it is that convergence, not any one chapter by itself, that this book is ultimately asking the reader to weigh.

Part IV — Language as Evidence

Chapter 9: The Substrate Argument

Every language that migrates into a populated territory tends to leave a specific kind of fingerprint: words for things the newcomers didn't have a name for, borrowed from the people who were already there. English is full of this pattern from its own layered history — Celtic river names surviving under a Germanic-then-Norman-French language stack, "bungalow" and "shampoo" arriving with British colonial contact in South Asia. Historical linguists have a name for the donor language in such cases even when it can't be identified or reconstructed: a substrate, a linguistic layer beneath the dominant language, detectable through the words it leaves behind even after the substrate language itself has vanished without a trace of its own.

The Rigveda carries exactly this kind of fingerprint. Linguists working on Rigvedic vocabulary have identified roughly 380 words in the text that do not fit comfortably into the Indo-European family at all — not merely words lacking obvious cognates in other IE languages, which could simply mean a cognate hasn't survived elsewhere, but words that actively violate the phonological and structural rules that reconstructed Proto-Indo-European root formation reliably follows. PIE roots are reconstructed as conforming to fairly strict templates — patterns of which consonants can appear in which positions, which sound clusters are permitted, which are not. Words that break those templates, cluster by cluster, are words that most likely were not inherited from PIE at all, but borrowed into Vedic Sanskrit from a different, non-Indo-European language already present in the landscape the newcomers entered.

Term	Meaning	Phonetic Violation / Linguistic Marker
Lāṅgala	Plough	Non-IE structural format; agricultural substrate vocabulary
Balbūtha	Name of a chief	Presence of 'b' (rare in inherited PIE roots); non-IE suffix *-ūtha*
Pramaganda	Tribal leader	Violates IE root structure; non-IE suffix *-anda*
Kosala	Region name	Violates the RUKI sound rule governing which sibilant is permitted after *i, u, r, k*
Śambara	Name of an enemy chief	Non-IE personal name; no attested Iranian or wider IE cognate

A few of these entries carry particular weight beyond their sheer number. *Lāṅgala*, "plough," is basic agricultural vocabulary — precisely the kind of word a population tends to borrow when it encounters an existing farming culture and its existing farming tools, rather than a word likely to be independently coined or inherited from a pastoralist ancestral vocabulary. And *Balbūtha* and *Pramaganda* are proper names — a chief and a tribal leader, individuals the hymns treat as real figures encountered by the Rigveda's own protagonists, whose names simply do not sound Indo-European by the reconstructed rules of the language family. A tradition that names, as historical or quasi-historical figures, people whose own names don't fit its own language's sound system is a tradition recording contact with a population that was not linguistically Indo-European — which is a very different picture from one in which the Rigveda's speakers had always been the only population in the landscape they describe.

> **The Other Case.** Substrate identification is not an exact science, and it would be dishonest to present every one of these 380-odd words as equally certain. Some historical linguists have challenged individual substrate etymologies proposed over the decades, arguing that a given "irregular" word could instead reflect an internal Sanskrit dialectal variant, an analogical reformation, or simply an incompletely understood piece of inherited vocabulary rather than a genuine outside loan — and it is true that not every substrate claim in the wider literature commands unanimous agreement. Indigenist scholars have specifically argued that the entire substrate-word program rests on circular reasoning: words get labeled "non-Indo-European" partly *because* they don't fit assumed migration-era timelines, then get cited as evidence for those same timelines.
>
> The response to that objection is not to claim every individual word is beyond dispute — several probably are debatable in isolation — but to point to the aggregate pattern rather than any single entry. The substrate hypothesis's evidentiary strength comes from convergence across independent structural markers: violation of established PIE root-formation templates, presence of sounds and sound-clusters rare or absent in securely reconstructed PIE vocabulary, non-Indo-European derivational suffixes, and a thematic clustering around agriculture, place names, and personal names — exactly the semantic domains substrate borrowing predicts. A skeptic can reasonably chip away at individual entries in that list of roughly 380 words. Explaining the aggregate pattern away in its entirety — the consistent phonological and semantic clustering across hundreds of independent words — requires more than isolated alternative etymologies; it requires an alternative account of why so many words cluster this particular way at once, and no such account has displaced the substrate hypothesis as the field's working consensus.

Chapter 10: Temperate Words in a Tropical Land

Reconstructed Proto-Indo-European vocabulary includes a specific cluster of words for plants and animals that share one thing in common: they belong to a temperate, not a tropical, ecosystem. The birch tree. The wolf. The beaver. The salmon. Snow, in more than one grammatical form. These words reconstruct cleanly across multiple Indo-European

branches — meaning they were very likely part of the language family's shared inherited vocabulary before it began splitting into the daughter branches (Germanic, Celtic, Slavic, Indo-Iranian, and the rest) that eventually spread across Eurasia. And versions of several of them survive, as recognizable cognates, inside Sanskrit and the wider Indo-Iranian branch — deployed in a linguistic environment describing the tropical and subtropical ecology of the Indian subcontinent, where wild salmon do not run and birch forests do not grow except at the very highest Himalayan elevations, far from where the Rigveda's geography is set.

The "Out of India" position has to explain this pattern somehow, because if Sanskrit and the wider Indo-European family radiated outward *from* India, the shared temperate vocabulary needs an explanation that doesn't simply say "the ancestral homeland was cold and northern" — because that ancestral-homeland reading points directly away from a tropical point of origin. The most common indigenist response is that vocabulary can persist as an archaism, applied loosely or metaphorically to different regional referents, without implying the speakers ever lived somewhere the original referent actually existed — much as English speakers in Australia and North America use inherited words like "robin" for birds that are only distantly related to the European robin the word originally named.

That is a real linguistic phenomenon, and it deserves acknowledgment rather than dismissal — words absolutely can be repurposed to describe locally available substitutes for an ancestrally named referent. But it is a weaker explanation here than it looks at first, for a reason specific to this case: the "robin" pattern typically involves colonizers applying an old word to a genuinely analogous *new* referent they actually encounter in their new environment — an American robin does resemble a European one enough to explain the reuse. The reconstructed PIE temperate vocabulary set as it survives into Indo-Iranian doesn't show that same substitution pattern nearly as consistently; several of the terms persist with their original temperate referent still recognizable in cognate meaning across branches, rather than being reassigned wholesale to unrelated tropical equivalents the way "robin" was reassigned. And this vocabulary pattern doesn't stand alone — it sits inside a wider comparative Indo-European family tree whose branching order (which branches share innovations with which neighbors, and in what sequence) independently and consistently points toward a homeland north and west of India, most commonly reconstructed somewhere in the Pontic-Caspian steppe region, rather than toward South Asia as the point of origin from which the rest of the family fanned out.

> **The Other Case.** It is worth being explicit that steppe-homeland reconstructions are themselves an active research area with real internal disagreement — the Pontic-Caspian steppe hypothesis (sometimes called the Kurgan hypothesis) is the current mainstream position, strengthened considerably in the past decade by ancient-DNA evidence tracking population movement out of the steppe region into both Europe and South Asia, but it is not the only homeland model that has ever been proposed, and debate over the finer details of timing and route continues within the mainstream position itself. This book is not claiming the homeland question is settled down to the decade or the specific archaeological culture. It is claiming something narrower and better supported: that whatever the precise homeland turns out to be, the accumulated linguistic and genetic

evidence converges on somewhere outside the Indian subcontinent, and converges against South Asia as the point of family-wide origin — which is the specific claim the "Out of India" position needs to overturn, and which the temperate-vocabulary pattern, read alongside the branching structure of the family tree, continues to weigh against rather than for.

Chapter 11: Beaver to Mongoose

Some linguistic evidence for migration is structural and statistical — patterns across hundreds of words, branching trees, sound-law regularities. Some of it is a single word, caught in the act of changing what it means as the people using it moved somewhere new. The *babhru*/*baβri* pair is the second kind, and it is one of the more vivid pieces of evidence available for the Indo-Iranian migration into South Asia precisely because it is so specific and so hard to explain any other way.

In the Avesta, the Zoroastrian scripture composed in the closely related Iranian branch of the family, the goddess Anāhitā is described in one striking passage as wearing a garment made from thirty beaver skins — the word used is *baβri*, directly cognate with the Sanskrit *babhru*. This is precise, concrete testimony: the Avestan tradition's speakers knew the beaver as a real, tangible animal, valuable enough to be woven into a description of divine luxury, familiar enough to name specifically rather than describe generically. Beavers are a northern, cold-climate, freshwater animal — they do not, and never did, live in the geographic range the Vedic hymns describe.

In Sanskrit, the cognate word *babhru* survives, but its meaning has shifted. Rather than continuing to denote the beaver — an animal the Vedic-period population, arriving in South Asia, would no longer have encountered — the word comes to denote the mongoose, a small, brownish, roughly similarly-colored carnivore that actually does live in the Indian subcontinent, and that a newly arrived population, reaching for a familiar word to describe an unfamiliar-but-visually-similar local animal, could plausibly have reassigned it to.

This is precisely the kind of semantic drift linguists look for as a fossil of migration: a word's *sound* survives essentially unchanged across two related languages, but its *referent* shifts specifically in the direction of "the closest available local substitute for a species the migrating population left behind." It is difficult to construct an alternative explanation for this particular shift that doesn't involve a population moving out of a beaver's range and into a mongoose's range while continuing to speak closely related dialects of the same ancestral language. The word itself is small. What it implies about the population carrying it is not: a group that once lived somewhere cold enough and northern enough for beavers to be a familiar, valuable animal, whose descendants ended up somewhere warm enough and southern enough that the same inherited word had to be pointed at something else entirely.

Combined with the temperate-vocabulary pattern from the previous chapter and the substrate-borrowing pattern from Chapter 9, the *babhru*/*baβri* case completes a specific kind of triangulation: not just "the ancestral vocabulary sounds northern" in the abstract, but a single traceable word caught mid-transition, still readable in both its old meaning (in

Avestan) and its new one (in Sanskrit), preserved by the accident of two closely related textual traditions surviving long enough for both snapshots to be compared side by side.

Part V — Method and Motive

Chapter 12: The Bardic Tradition

Not all of the Vedic corpus was composed, or transmitted, the same way — and the difference matters enormously for how much historical weight any given passage can bear. The Rigveda Samhita, the oldest layer, was preserved through a system of oral transmission of extraordinary rigor: reciters memorized not just the words but multiple parallel recitation methods designed specifically to detect and prevent any drift in the text over generations of purely oral transmission, a process sometimes called orthoepic diaskeuasis — meticulous, self-correcting preservation, engineered against exactly the kind of gradual embellishment and legendary accretion that oral traditions are normally vulnerable to.

The later strata of the wider Vedic-adjacent textual universe — the Brahmanas, and further out, the Epics (the Mahabharata and Ramayana in their developed forms) and the Puranas — were not preserved under anything like the same disciplined constraint. These are *Smṛti* texts, "what is remembered" rather than *Śruti*, "what is heard" (revealed, and preserved with Samhita-level rigor), and they were composed, redacted, and re-redacted across many centuries, absorbing legendary elaboration, regional variation, and evolving theological emphasis as they went. This is not a criticism of their literary or religious value — the Puranic tradition is a vast and significant achievement in its own right. It is a statement about their reliability as a source for reconstructing the physical geography and material culture of the third millennium BCE, a period many centuries before most of this later material took the form it now has.

The "Sindhu-Sarasvatī" rebranding's most vivid imagery — a mighty, sea-going, civilisation-defining Sarasvatī river — draws disproportionately on exactly this later, looser textual layer: Puranic and epic descriptions of the Sarasvatī's grandeur, composed and elaborated many centuries, in some cases well over a millennium, after the Rigveda Samhita's own composition, and further still from the Bronze Age archaeology the rebranding is trying to characterize. Using a bardic, evolving, legend-accreting tradition's description of a river to date and characterize an urban civilisation two to three thousand years earlier is a category error familiar to historians from many other contexts: it is roughly analogous to reconstructing the physical geography of Iron Age Britain from the description of Camelot in a medieval Arthurian romance, because the romance happens to be set, nominally, in the same island.

This is not a claim that the later tradition's veneration of the Sarasvatī is meaningless — cultural memory of a once-great river, even one that had already substantially diminished by the time of the earliest Rigvedic composition, plausibly grew rather than shrank in successive retellings, exactly the pattern legendary elaboration typically follows. It is a claim about what kind of evidence that veneration constitutes. It is strong evidence for how deeply the Sarasvatī mattered to the cultural imagination across many centuries of Indian religious history. It is weak evidence — evidence that needs independent corroboration before it can bear real weight — for the specific physical and chronological claim that the river was navigably sea-going and civilisation-defining at the precise moment, three thousand years earlier, that Harappan cities stood along the Indus.

Chapter 13: The Status Kit Model

Older popular accounts of the Indo-Aryan migration into South Asia often reached for a simple, dramatic image: invasion, conquest, a wave of chariot-riding warriors sweeping in and displacing or subjugating an existing population by force. That image has largely fallen out of favor among specialists, not because the migration itself has been abandoned as a hypothesis, but because the "invasion" framing turned out to demand a kind of evidence — mass destruction layers, widespread skeletal trauma, dramatic population replacement — that the archaeological and, more recently, the genetic record simply does not show at anything like the scale the invasion narrative implies.

What has replaced it in current scholarship is closer to a model developed by historians and archaeologists including David Anthony and Christopher Ehret, sometimes described as an elite-dominance or "status kit" model. The core idea is straightforward: a relatively small, mobile population, possessing a specific bundle of prestige technology and cultural practice — in this case, the horse, the chariot, associated ritual complexes, and a particular social organization built around them — can spread its language and much of its cultural framework across a much larger existing population without needing to replace that population demographically or genetically at all. Language and prestige culture can be adopted by existing local populations from the top down, as elites intermarry, as local leaders adopt the newcomers' more prestigious technology and the social status that comes with it, and as language shifts generationally follow status rather than violence.

This model has clear historical analogues outside South Asia, cases where the evidence trail is comparatively unambiguous. The Magyars who settled in the Hungarian basin left a lasting linguistic imprint — Hungarian remains a linguistic island, unrelated to its Slavic and Germanic neighbors — while contributing only a modest fraction to the region's overall genetic ancestry; most of the population's genetic ancestry continued to derive from the pre-existing Carpathian Basin population. The Bulgars, similarly, gave their name and initial ruling structure to what became Bulgaria while being linguistically and largely genetically absorbed into the Slavic-speaking population they came to rule. In both cases, a small, technologically or militarily advantaged elite reshaped a region's language and political identity while leaving a comparatively light genetic footprint — exactly the pattern the status-kit model predicts, and exactly the pattern that has made "why don't Indians simply look like a displaced population" — a question the older invasion model struggled to answer convincingly — no longer a serious objection to Indo-Aryan migration once the correct model is applied.

This matters directly for the "Sindhu-Sarasvatī" debate because it removes the strongest emotional objection to the migration hypothesis: the fear that "migration" is a coded way of saying today's Indian population is somehow not really indigenous, or is descended primarily from foreign invaders who displaced an earlier population. The status-kit model implies close to the opposite. It suggests a comparatively small incoming population whose language, ritual practice, and elite social structure spread widely through adoption and intermarriage across an already large, already resident population — a population whose genetic ancestry remains overwhelmingly continuous with the subcontinent's deep prehistory, with a real but modest and traceable steppe-related genetic component layered

on top, a pattern increasingly confirmed by the ancient-DNA studies of the past decade. Indo-Aryan migration, on this model, is a story about how a language and a religious-cultural framework spread — not a story about one population being replaced by another. That distinction resolves a great deal of the political heat this topic generates, once it is actually stated rather than left implicit.

Chapter 14: Scientific Cultism

Satellite imagery has a particular rhetorical power that is worth examining directly, because the "Sindhu-Sarasvatī" rebranding leans on it heavily. A LANDSAT photograph of a paleochannel — the faint, unmistakable trace of an ancient riverbed, visible from orbit even where no water has flowed on the surface for millennia — looks like unambiguous, unmediated proof. It is a picture, not an argument; it seems to speak for itself, and it carries the unearned authority that images from space tend to carry in popular science communication generally. This chapter's argument is not that the imagery is fake or that the paleochannels it reveals are not real. They are real, and the underlying geomorphological finding — that the Ghaggar-Hakra system was once a substantially larger river than it is today — is not seriously disputed. The argument is about what kind of question satellite imagery can actually answer, and what kind of question it is being used to answer here.

A paleochannel visible from orbit tells you that a river of a certain size once flowed along a certain course. It does not, by itself, tell you *when* — not with the precision the rebranding's claim requires. Dating a paleochannel's active-flow period requires ground-truthing: stratigraphic excavation, radiocarbon dating of organic material recovered from the channel deposits, optically stimulated luminescence dating of the sediment itself, sometimes years of fieldwork layered on top of the initial satellite identification. Satellite imagery is where the geological investigation *starts*. Presenting it as though it were where the historical argument *ends* — as though the mere existence of a visible ancient riverbed settled the question of whether that river was flowing, at civilisation-defining volume, during the specific centuries the Rigveda was being composed — collapses several separate steps of evidentiary work into a single, visually persuasive image.

This chapter's title borrows a slightly provocative phrase deliberately: "scientific cultism" describes not any individual scientist's work, most of which is careful and appropriately hedged in its original publication, but a specific rhetorical strategy that appears downstream of that careful work — the strategy of reaching for the trappings of hard science (satellite data, geological terminology, the visual authority of an orbital photograph) to lend an unearned sense of settled empirical proof to what remains, underneath the imagery, a contested historical and chronological argument. It is a pattern that recurs across many politicized historiographical debates worldwide, not uniquely an Indian phenomenon: borrow the aesthetic and vocabulary of a harder, more settled science to dress up a genuinely disputed humanities question as though the dispute had already been resolved by instrumentation.

The corrective is not to distrust satellite archaeology — it is a genuinely valuable and increasingly important tool, and the paleochannel mapping of the Ghaggar-Hakra system has meaningfully advanced the field's understanding of the region's ancient hydrology. The

corrective is to keep the different evidentiary steps visibly separate in any argument that uses them: what the imagery shows (a river once flowed here, at some point, at some scale), what stratigraphic and dating work independently establishes (roughly when it was flowing at that scale, and roughly when it stopped), and what remains a separate, comparative-linguistic question entirely (when the Rigveda was composed, and whether that composition window overlaps with the dated hydrology). Collapsing all three into a single photograph is where the argument goes from geology to advocacy without announcing the transition — and a textbook, of all documents, owes its reader that announcement.

Part VI — Other Erasures

Chapter 15: The Baudhayana-Pythagoras Question

Every so often, a claim circulates in Indian classrooms, science exhibitions, and social media threads that the theorem the world knows by a Greek philosopher's name was actually stated first, centuries earlier, by an Indian sage named Baudhayana. This claim is not folklore. The Baudhayana Shulba Sutra, a ritual manual for constructing Vedic fire altars composed roughly around 800 BCE, contains a general geometric rule — that the diagonal of a rectangle produces a square whose area equals the sum of the squares on the two sides — stated clearly enough, and applied practically enough in the altar-construction instructions that follow it, that historians of mathematics do not dispute the priority claim in outline. Pythagoras, by the most common dating, lived roughly 570–495 BCE, two to three centuries after the Shulba Sutra's likely composition. Calling the result the "Baudhayana theorem" rather than, or alongside, the Pythagorean theorem is not a nationalist exaggeration. It is a reasonably defensible correction to a Eurocentric naming convention, and this chapter is not here to dispute it.

What this chapter is here to examine is a pattern, not a single claim: which ancient Indian mathematicians get remembered, retold, and celebrated in exactly this way, and which do not — and whether that selection tracks anything other than the underlying mathematics itself.

Baudhayana belongs to a well-known lineage that Indian popular science writing, museum placards, government commemorations, and school-level enrichment material return to reliably: Vedic and Vedic-adjacent figures — Baudhayana and the other Shulba Sutra authors, Aryabhata, Brahmagupta, Varahamihira, Bhaskara I and II — each with a stable, frequently repeated story (Aryabhata and the zero, Brahmagupta and negative numbers, Bhaskara and early calculus-adjacent ideas) that circulates well beyond specialist history-of-mathematics circles into general civic pride. This is not, in itself, a problem. These are real mathematicians who did real, often extraordinary work, and a country taking pride in that work is unremarkable.

The problem is what sits just outside the frame of that story, largely unmentioned in the same textbooks, the same museum placards, and the same popular retellings — despite working in the same broad tradition, in some cases in the very same royal courts, within decades of the more famous names.

Acharya Virasena, a Digambara Jain monk who lived roughly 792–853 CE in the Deccan under the patronage of the Rashtrakuta king Amoghavarsha, completed in 816 CE a mathematical commentary called the Dhavala, on the Jain canonical text Shatkhandagama. In it, Virasena develops and names the concept of *ardhaccheda* — the number of times a quantity can be halved — which is, in substance, a base-2 logarithm, worked out with its governing laws centuries before John Napier's 17th-century treatise is conventionally credited, in most general histories of mathematics, as the origin of logarithmic thinking. Virasena went further, working out logarithms in base 3 (*trakacheda*) and base 4 (*caturthacheda*) as well, derived the volume of a conical frustum using an early

infinite-series procedure, and offered an approximation for pi — roughly 3.14159292 — measurably more precise than the value commonly attributed to Aryabhata's own treatise. None of this is obscure or contested among historians of Indian mathematics; it appears in standard references on the subject. It is simply, overwhelmingly, absent from the general-audience version of "India's ancient mathematical genius" that gets retold in classrooms and popular media.

Jinasena, Virasena's direct disciple, completed the Dhavala after his teacher's death and went on to author the Adipurana, part of the Mahapurana — continuing the same Rashtrakuta-court tradition of Jain scholarship that had produced his teacher's mathematics. And Mahaviracharya, writing a generation later at the same Rashtrakuta court under the same King Amoghavarsha, composed in 850 CE the Ganita Sara Sangraha — the earliest Indian text devoted entirely to mathematics as a subject in its own right, rather than as a chapter embedded within an astronomical treatise, as most earlier Sanskrit mathematical writing had been. Mahaviracharya systematized fractions, permutations and combinations, and quadratic problem-solving, established still-used geometric terminology for figures like the rhombus and isosceles triangle, and explicitly credited the Jain mathematical lineage he was building on in his own introduction. His work was influential enough to be translated into Telugu within roughly two centuries and to shape mathematical instruction across the south for generations. He is, by any ordinary measure of historical significance, at least as important a figure in the history of Indian mathematics as several names that appear far more often in general retellings.

What makes this pattern worth a chapter, rather than a footnote of grievance, is the specific historical irony sitting underneath it: Virasena, Jinasena, and Mahaviracharya were not marginal, geographically scattered figures whose obscurity might be explained by isolation from the mainstream of classical Indian scholarship. They worked, across three overlapping generations, at the same royal court — the Rashtrakuta court at Manyakheta, under a dynasty whose most celebrated ruler, Amoghavarsha, is recorded as having personally engaged with Jain scholarship and, by several traditional accounts, spent part of his later life in Jain monastic retreat. This was not a fringe tradition operating in the shadow of a Vedic mainstream; it was, for a concentrated and well-documented span of the 8th and 9th centuries, a state-patronized center of mathematical scholarship in its own right, producing work that in the specific case of logarithms and infinite series predates comparable European developments by the better part of a millennium.

None of this is difficult to find. It sits in the same standard reference works — the MacTutor History of Mathematics archive, peer-reviewed papers on the history of Indian science, dedicated Jain studies journals — that supply the Aryabhata and Brahmagupta material used in general textbooks. The information asymmetry is not a shortage of scholarship. It is a shortage of retelling: a default reflex, when the question "who discovered this in ancient India" comes up, to reach first and almost exclusively for a Vedic or Puranic-affiliated name, and to leave Buddhist, Jain, and other traditions' comparably significant contributions in the specialist literature rather than the general one.

This is the same historiographical habit this book has spent its preceding chapters examining in a different domain, and it is worth naming plainly rather than leaving implicit.

The Sindhu-Sarasvatī rebranding collapses a genuine, evidenced distinction between two cultures into a single, more comfortable continuity narrative. The mathematics-textbook version of the same habit does something structurally similar at smaller scale: it does not deny that Virasena or Mahaviracharya existed, any more than the rebranding denies the Indus cities existed. It simply keeps the spotlight trained on one strand of a demonstrably pluralistic intellectual history, generation after generation, until "ancient Indian mathematics" starts to sound, in the popular imagination, like a story with only Vedic and Puranic characters in it — a story India's actual historical record does not support.

For a country whose modern constitution explicitly protects religious pluralism, and whose population still includes millions of Jains, Buddhists, and other communities whose ancestors were not incidental bystanders to Indian civilisation but among its most sophisticated builders, that quiet narrowing matters beyond the history-of-mathematics seminar room. A curriculum that keeps defaulting to a single civilisational strand as the answer to "who were the ancient Indian geniuses" teaches, by omission, a subtly exclusionary answer to a much larger and more consequential question: who gets to count, in the story a nation tells its children, as a full author of that nation's greatness. Correcting Pythagoras's name to include Baudhayana's was the easy part of that reckoning, because it flattered a story India already wanted to tell about itself. Making room for Virasena, Jinasena, and Mahaviracharya in the same sentence is the harder part, because it asks the story to become more crowded, more plural, and less tidy than the version currently being taught — which is, this book would argue, the more accurate version, and the only one a genuinely diverse country can afford to keep telling itself.

Part VII — Stakes

Chapter 16: Summary of Contradictions

Sixteen chapters of evidence resolve into a comparatively simple picture, even though reaching it required tracing chariot wheels, horse bones, metallurgical vocabulary, river deltas, hydronyms, hymn geography, borrowed words, semantic drift, textual layering, migration modeling, and satellite methodology one theme at a time.

The Indus Valley Civilisation was an urban, planned, non-horse-using Bronze Age culture whose great cities had substantially declined, and in most cases been abandoned, by roughly 1900 BCE. Its material culture shows no confirmed archaeological trace of the horse-and-chariot complex that saturates the Rigveda's own self-description, and its faunal and iconographic record centers instead on cattle, consistent with an agrarian rather than a pastoral-martial economy.

The Vedic culture reflected in the Rigveda's earliest layers was a later-arriving, horse-centered, chariot-organized society, whose archaeological signature in South Asia begins no earlier than roughly 1700 BCE and whose own internal vocabulary — tracked through the *ayas* to *śyāma ayas* metallurgical shift — continued developing for centuries beyond that, well into the early Iron Age closer to 1000 BCE. This is not a culture contemporaneous with Harappan urbanism; on the weight of current chronological evidence, it is a culture that arrived afterward.

The river the rebranding's name invokes does not straightforwardly support the sea-going, civilisation-defining role assigned to it. The Ghaggar-Hakra system's most likely desiccation timeline runs earlier than the Rigveda's most probable composition window, and the river's name itself has a precise cognate on the Iranian plateau, terminating in an equivalent inland desert delta rather than a sea — a pattern more consistent with a migrating population carrying a familiar name to a new but hydrologically similar river than with an indigenous, unbroken naming tradition centered on a single sea-reaching waterway.

The language itself carries the clearest fingerprint of all: roughly 380 words in the Rigveda that do not fit the Indo-European sound system, clustering suspiciously around agriculture, place names, and the names of specific individuals encountered by the text's own protagonists; a reconstructed ancestral vocabulary for temperate flora and fauna sitting oddly inside hymns describing a tropical landscape; and a single word, *babhru*, caught mid-drift from "beaver" to "mongoose" as its speakers moved from a colder homeland into a warmer one.

And the rebranding's own methodology leans on a bardic, centuries-later textual tradition to characterize a Bronze Age archaeological reality, and on satellite photography to settle a chronological question that satellite photography alone cannot answer. None of this evidence, examined individually or in combination, supports collapsing the Indus Valley Civilisation and the Rigvedic Vedic culture into a single, continuously named entity. It supports, instead, the picture professional archaeology, comparative linguistics, and — increasingly — ancient-DNA research have converged on independently: two distinct

cultures, overlapping in geography but not in time, connected not by identity but by a later population's arrival into a landscape an earlier civilisation had already built, inhabited, and left behind.

Chapter 17: Why the Classroom Is the Battleground

Every nation with a long and contested past eventually faces a version of the question this book has spent seventeen chapters examining: who gets to decide what the deep past means, and where does that decision get made? The answer, more often than courtrooms or journals, is the classroom. History taught to a ten-year-old does not arrive with footnotes, competing interpretations, or a bibliography of dissenting scholarship. It arrives as fact, delivered by an authority figure, absorbed at an age when critical evaluation of sources is not yet a fully developed skill — and it tends to stay exactly that way, unrevisited, for the rest of that person's life. This is precisely why curriculum committees carry a different kind of responsibility than individual scholars publishing in a journal that only other specialists will read critically.

This is not a uniquely Indian problem, and it would be a mistake to read this book as though it were. Contested national origin stories, and the political stakes of teaching one version of them as settled fact, are a recurring feature of history education everywhere — from debates over how colonization is described in North American curricula, to disputes over the framing of national founding myths across Europe and East Asia, to earlier ideological disputes within India's own textbook history that this book flagged in Chapter 2. What makes the "Sindhu-Sarasvatī" case worth an entire book, rather than a footnote in a broader survey of textbook politics, is the unusual richness and cross-checkability of the evidence available to test it against: archaeology, comparative linguistics, zooarchaeology, hydrology, and ancient genetics all bear independently on the same underlying question, and — as the preceding sixteen chapters have tried to show — all of them currently point in a broadly consistent direction, away from the continuity claim the rebranding asserts. And as Part VI showed, the underlying habit of mind is not confined to this one civilisational question; it recurs wherever a curriculum reaches for a single, comfortable strand of ancient Indian identity and quietly lets the others go unmentioned.

None of this argues for teaching children that history is unknowable, or that every question is equally uncertain, or that nothing in a textbook can be trusted. Most historical claims genuinely are settled, in the ordinary sense that no serious specialist disputes them, and textbooks are right to teach those claims as fact without hedging every sentence into meaninglessness. The argument here is narrower and, this book hopes, more defensible: where a claim is genuinely contested among specialists — as the relationship between the Indus cities and the Rigvedic culture demonstrably is — a curriculum has an obligation to say so, rather than resolve the contest silently by embedding one side's conclusion into a compound proper noun that a student has no tools to question. "Some scholars believe X; others argue Y, based on Z evidence" is not a failure of pedagogical confidence. It is what honest teaching of a genuinely open question looks like, and it is a sentence structure Indian textbooks, like textbooks everywhere, are entirely capable of using when the writers choose to.

The stakes of getting this right extend beyond any one civilisation's name. A society that grows comfortable resolving contested historical questions by curricular fiat — on any side of any given debate — is a society training its next generation not to ask what the evidence shows, but to accept what the textbook states. That habit, once formed in the history classroom, does not stay confined to ancient rivers and Bronze Age chariots. It is the habit this book has tried, chapter by chapter, theme by theme, to push back against — not by asking readers to simply believe a different settled story instead, but by showing, at every step, the specific evidence a reader could go and check for themselves.

Further Reading

This book argues one side of a genuine scholarly debate. Readers who want to weigh the fuller range of positions, including those this book has been critical of, may find the following starting points useful:

- David W. Anthony, *The Horse, the Wheel, and Language* (2007) — the fullest development of the steppe-homeland and elite-dominance migration model referenced throughout Part V.
- Asko Parpola, *The Roots of Hinduism: The Early Aryans and the Indus Civilization* (2015) — a detailed archaeolinguistic treatment of the Indus-to-Vedic transition from a migrationist perspective.
- Edwin Bryant, *The Quest for the Origins of Vedic Culture: The Indo-Aryan Migration Debate* (2001) — a deliberately even-handed survey of both the migrationist and indigenist positions, useful precisely because it does not argue for either side.
- Michael Witzel's published work on Rigvedic substrate vocabulary, referenced throughout Part IV, for readers who want the linguistic argument in full technical detail.
- B. B. Lal's later writings represent one of the more prominent indigenist counter-positions in Indian archaeology and are worth reading directly by anyone who wants the strongest available version of the "Other Case" sections in this book, argued at full length rather than summarized.

This list is a starting point, not an endorsement of every claim in every listed work — including, in places, this book's own. Readers are encouraged to hold this book to the same standard it has tried to hold the curriculum it examines.